

was taken by Profs. Tait and Dewar (July, 1875), who showed how the increase, resulting from rarefaction, in the mean length of the path of the gaseous molecules would favour the action, but the explanation in the form which they gave to it required that the rarefaction should be carried far enough to make the mean length of path of a molecule of gas great as compared with the dimensions of the inclosing vessel. It has, however, been pointed out by Prof. Zöllner (*Pogg. Ann.*, February, 1877), and more recently by Mr. Tolver Preston (*Phil. Mag.*, August, 1877), that, in the majority of cases, this condition is far from being fulfilled. On the other hand, the residual-gas theory of the action of the radiometer received very important experimental support from Dr. Schuster's beautiful demonstration (February, 1876) that the force exerted on the discs was correlative with an equal opposite force exerted upon the glass envelope. The complete proof that the action was due in some way to the presence of residual gas was furnished by Mr. Crookes's own discovery (June, 1876) that it rapidly diminishes when the exhaustion is carried beyond a certain point depending on the nature of the gas. The outstanding defect in the theory was removed by Mr. Johnstone Stoney, who (*Phil. Mag.*, March and April, 1876) showed that the observed phenomena might arise at a degree of rarefaction at which the mean length of path of a molecule was still much below the distance from the discs to the envelope, it being sufficient that this distance should not be too great to allow the warming of the discs to cause a sensible increase in the velocity with which the molecules struck the glass. Mr. Stoney's form of the theory answers to all the facts of the case, so far as I am acquainted with them, and it has been confirmed and illustrated by Mr. Crookes with a numerous series of remarkably beautiful and ingenious experiments.

My object in thus tracing the chief stages in the growth of the accepted theoretical explanation of the radiometer has been to point out that the quality of mind which led Mr. Crookes to reject the various suggested explanations of the phenomena he had observed, so long as they were only approximate and did not account for *all* his facts, was merely a further exemplification of the quality which led him to the original discovery. If he had been content to disregard a seemingly trivial fact he would never have made this discovery at all, and if he had disregarded slight defects in the explanations that were offered he would have missed some of its most important consequences. I think that this also might have been suitably included among the "Lessons of the Radiometer."

G. CAREY FOSTER

University College, London, October 27

HAS Dr. Carpenter allowed himself to become possessed by a "dominant idea?" From his letter in NATURE (vol. xvi. p. 544), I infer that he *might* have taken the trouble to reply to my article in the July number of the *Nineteenth Century*, had he not thought that my assertions "were well known in the scientific world to be inconsistent with fact."

Some remarks, however, made by Prof. G. Carey Foster at the British Association seem to have forced upon Dr. Carpenter the conviction that he may have underrated my character for veracity, and that the "scientific world," at all events, is not unanimous in regarding my "assertions" as falsehoods. Dr. Carpenter therefore seeks in your columns to justify the statements contained in his article on "The Radiometer and its Lessons," in the *Nineteenth Century* for April last.

When Dr. Carpenter declares my "assertions (1) . . . (2) . . . (3)" to be false, I have a right to demand that Dr. Carpenter give my identical words, and not his own interpretation of my words—an interpretation which is "inconsistent with fact."

To show Dr. Carpenter's inaccuracies in small things as well as great, I may point out that he does not even quote correctly the title of my article in the *Nineteenth Century*. His carelessness in more important matters is of deeper consequence. In order to enforce one of his dominant ideas "yet more fully and emphatically," he tells us that he applied himself to a "careful reperusal of" my papers "with the most earnest desire to present a true history of the whole inquiry." A most laudable determination! And where, will it be believed, did Dr. Carpenter, a Fellow of the Royal Society, go for information? To the *Philosophical Transactions*, where my papers are printed at full length? No! He only referred to the "*Proceedings of the Royal Society*," a record, as every one knows, that contains brief, and therefore imperfect abstracts of what is published in full in the *Transactions*.

In his "justification" Dr. Carpenter quotes a passage from a lecture I delivered in 1874, on The Repulsion Accompanying Radiation, commencing, "my own impression is," &c. Had Dr. Carpenter quoted the next paragraph, which is necessary to a correct interpretation of the sentence he did quote, your readers would have been enabled to judge how far I advanced theories of my own. My words were these: "I do not wish to insist upon any theory of my own. . . . The one I advance is, to my mind, the most reasonable, and, as such, is useful as a working hypothesis, if the mind must have a theory to rest upon. Any theory will account for *some* facts, but only the true explanation will satisfy *all* the conditions of the problem, and this cannot be said of either of the theories I have already discussed." My next paragraph concludes with the following quotation from Sir Humphry Davy:—"When I consider the variety of theories which may be formed on the slender foundation of one or two facts, I am convinced that it is the business of the true philosopher to avoid them altogether. It is more laborious to accumulate facts than to reason concerning them; but one good experiment is of more value than the ingenuity of a brain like Newton's."

With regard to my having "theorised on the subject," I have never denied having done so, although I have on five or six occasions specially stated that "I wished to keep free from theories," and "unfettered by the hasty adoption" of theories. But I do deny that I ever stated that my results were definitely explained by the direct mechanical action of light. Your readers will understand that an experimental research is necessarily and slowly progressive, and that the early provisional hypotheses have to be modified, and perhaps altogether abandoned, in deference to later observations. Until my experiments confirmed the explanation given by Mr. Johnstone Stoney, I adopted no definite theory, and I contend that a trained physicist would fail to gather from my published papers that I desired my first impressions to be regarded as final.

Dr. Carpenter again attributes to me the terms "a new force," or a "new mode of force," as applied to the repulsion accompanying radiation. Unless Dr. Carpenter can point these words out in my published papers, he has no right to place them between inverted commas.

But the chief burden of Dr. Carpenter's song is that "Mr. Crookes has another side to his mind, which makes Mr. Crookes the spiritualist almost a different person from Mr. Crookes the physicist." I fail to see how the investigation of certain phenomena called spiritual can make a man a spiritualist, even if he comes to the conclusion that some of the phenomena are not due to fraud. My position in this matter was clearly stated some years ago, and I ask your permission to quote the following passages from an article I published in 1871:—"I have desired to examine the phenomena from a point of view as strictly physical as their nature will permit. . . . I wish to be considered in the position of an electrician at Valencia examining, by means of appropriate testing instruments, certain electrical currents and pulsations passing through the Atlantic cable; independently of their causation, and ignoring whether these phenomena are produced by imperfections in the testing instruments themselves, whether by earth currents or by faults in the insulation, or whether they are produced by an intelligent operator at the other end of the line."

From this stand-point I have never deviated. Can Dr. Carpenter say that his position and mine, in respect to the investigation of the phenomena ascribed to spiritualism, are so very different? He asserts that he has shown beyond doubt that it is all imposture. But I would ask if this was proved to his satisfaction twenty years ago, why does he still waste valuable time in interviews and sittings with so-called mediums? If I am to be censured for having devoted time to this subject, such censure must be doubly applicable to a man who commenced the investigation when I was a child, and who cannot let the subject drop whenever a new "medium" comes in his way. Does he regard the subject as his own special preserve, and may his demonstrations against other explorers in this domain of mystery be looked upon as the conduct of a gamekeeper towards a suspected poacher?

To impress on the world that he has no "*animus*," Dr. Carpenter says he "cordially" and "personally congratulated" me. His words bring vividly to my mind the conversation, of which, by the by, he has omitted an important part. It was at the annual dinner of the Fellows of the Royal Society on November 30, 1875, when the royal medal was awarded to me. Dr. Carpenter accosted me with great apparent cordiality, and said,

"Let us bury the hatchet! Why should scientific men quarrel?" I signified my full acceptance of the offered peace, and great was my surprise soon after to find that, unmindful of the understood compact, he had exhumed his hatchet and was dealing me unexpected and wanton strokes, tempered by a certain amount of half praise which reminds me of the sort of caressing remonstrance of Majendie in the pre-anæsthetic days, to the dog which he had on his operating table—"Taisez vous, pauvre bête!"

In all seriousness, however, I must again ask, what is the meaning of the "personal antagonism," and the persistent attacks which Dr. Carpenter, for the last six years, has directed against me? In his recently published book, in the *Nineteenth Century*, and in his last letter to you, the key-note struck in the *Quarterly Review* six years ago is sustained. We have the same personalities, the same somewhat stale remark about my double nature, and the same exuberance of that most dangerous and misleading class of averments, half truths. Dr. Carpenter, indeed, condescends to admit that I have pursued "with rare ability and acuteness a delicate physical investigation in which nothing is taken for granted without proof satisfactory to others as well as to himself," and that I have "carried out a beautiful inquiry in a manner and spirit worthy of all admiration;" but, after granting so much, he dissembles his love and proceeds to "kick me down stairs." I am damned with faint praise, and put to rights in such a school-masterly style, that I could almost fancy Dr. Carpenter carries a birch rod concealed in his coat-sleeve. He admits that in an humble and subordinate sphere I have done useful work, only I must not give myself airs on that account. Dr. Carpenter reminds me of Dr. Johnson defending Sir John Hawkins, when he was accused of meanness. "I really believe him," said Johnson, "to be an honest man at the bottom; but to be sure he is penurious, and he is mean, and it must be owned he has a degree of brutality, and a tendency to savageness, that cannot easily be defended." In the same magnanimous spirit Dr. Carpenter allows that I have contributed a trifle to science, but he does not forget to add that I am the victim of cerebral duplicity, and I am again held up to illustrate the sad result of neglecting to train and discipline "the whole mind during the period of its development," &c.

I have, it appears, two allotropic personalities, which I may designate, in chemical language, Ortho-Crookes and Pseudo-Crookes. The Ortho-Crookes, according to Dr. Carpenter, has acquired "deserved distinction as a chemist." He carries out a "beautiful inquiry in a manner and spirit worthy of all admiration." He has shown "ability, skill, perseverance, and freedom from prepossession." He pursues "with rare ability and astuteness a delicate physical investigation." He evinces the "spirit of the true philosopher," and he has "deservedly" received "from the Royal Society the award of one of its chief distinctions."

But Pseudo-Crookes, whose career Dr. Carpenter has evidently watched almost from his cradle—as he professes to know the details of his early education—unfortunately took a "thoroughly unscientific course," and developed into a "specialist of specialists." He had "very limited opportunities" and "never had the privilege of associating" with scientific men, although he displayed "*malus animus*" "towards those with whom he claims to be in fraternity." He is "totally destitute of any knowledge of chemical philosophy, and utterly untrustworthy as to any inquiry" not technical. His "assertions" are "well known in the scientific world to be inconsistent with fact." He enters on inquiries "with an avowed foregone conclusion of his own." He has "lent himself to the support of wicked frauds." He has "prepossessions upon which clever cheats play." His "scientific tests" are not "worthy of trust." He is a believer in "day dreams," and the supporter of a "seething mass of folly and imposture;" whilst, to crown all, he actually thinks that the radiometer is driven "by the direct impetus of light." In short, this Pseudo-Crookes is a compound of folly and knavery such as has rarely, if ever, previously been encountered.

WILLIAM CROOKES (The Ortho-Crookes?)

London, October 29

Mr. Wallace and Reichenbach's Odyle

I AM amazed that Dr. Carpenter should think it necessary to make public, with such haste, Prof. Hoffmann's statement that Baron Reichenbach's facts and theories are not accepted by the

body of scientific men in Germany. Of course they are not. But how this affects their intrinsic accuracy I fail to see. Less than twenty years ago the scientific men of all Europe utterly disbelieved in the co-existence of man with extinct animals; yet the facts adduced by Freere, Boué, McEnery, Godwin Austen, Vivian, and Boucher de Perthes, are now admitted to have been trustworthy and deserving of the most careful examination. The whole history of scientific discovery from Galvani and Harvey to Jenner and Franklin, teaches us, that every great advance in science has been rejected by the scientific men of the period, with an amount of scepticism and bitterness directly proportioned to the novelty and importance of the new ideas suggested and the extent to which they run counter to received and cherished theories. Rejection is one thing, disproof is another; and I have in vain searched for anything like disproof, or even rational explanation, of Reichenbach's facts; his theory, or "Odyle-doctrine," I have never "attempted to rehabilitate," as Dr. Carpenter, with his usual misconception, says I have done. In my review of Dr. Carpenter's lectures (*Quarterly Journal of Science*, July, 1877, p. 396), I adduce five tests employed by Reichenbach, and also the independent and simultaneous confirmation of Dr. Charpignon in France; and the only reply I get is: "All men of science disbelieve them." With the facts of history above alluded to in my mind, and believing that human nature is very much the same in the nineteenth century as it was in the eighteenth, I can only say, "so much the worse for the men of science."

Dr. Carpenter's reference to the believers in a flat earth, as a parallel case, is unfortunate, because the two cases are really of a totally different nature. Those who maintain the earth to be flat do not deny the main facts which we rely on as proving it to be round, but they attempt to give other explanations of them. The dispute is on a question of reason and inference; and every intelligent and fairly educated man is able to decide it for himself. But in Reichenbach's case it is the *facts* that are rejected without disproof or adequate explanation. The two cases are therefore quite distinct, and Dr. Carpenter's attempted parallel, as well as his setting up of scientific disbelief as a conclusive reply to evidence, is in conformity with his whole treatment of this subject.

I trust that such of the readers of NATURE as may feel any interest in the questions at issue between Dr. Carpenter and myself will read my article above referred to, and not allow themselves to be influenced by Dr. C.'s repeated appeals to authority and to prejudice.

ALFRED R. WALLACE

I HAVE to request your insertion of a post-card I have this morning received, for two reasons; *first*, because, as it is anonymous, and as the writer of it is obviously a reader of NATURE, no other way is open to me for replying to it except that which your columns may afford; and *secondly*, because it is a very curious example of the misconceptions into which men are apt to fall who allow themselves to become "possessed" by "dominant ideas."

"If Mr. A. R. Wallace has to choose between being either 'a fool or a knave,' there is *at all events no choice left for the man* who deliberately and maliciously makes incorrect assertions and suppresses the truth to further his own views. I dare say you know what most people would call such a man. Yours,

"ONE WHO WAS AT PLYMOUTH"

Now, in the first case, it must be perfectly obvious to any one who is capable of reasoning logically, that nothing which I said of Mr. Wallace in your last number can be twisted into the implication that he is either "a fool or a knave." John Hampden is continually saying this of Mr. Wallace and of everybody who upholds the rotundity of the earth. And I mildly suggested whether, in putting himself in opposition to the whole aggregate of scientific opinion on the value of Reichenbach's Odylism—not because he had himself repeated them, but because he believes in Reichenbach—Mr. Wallace is not assuming an attitude in some degree similar, that is, setting himself up as the one wise and honest man who duly appreciates Reichenbach, and therefore implying that everybody else is either stupidly or wilfully blind to the evidence he presented. If anyone thinks it worth while to read Mr. Wallace's review of my lectures on "Mesmerism, Spiritualism," &c., in the last number of the *Quarterly Journal of Science*, he will be able to judge whether I have or have not wronged Mr. Wallace in this matter.

The writer's appreciation of my own character, which has fre-